# Cultural and Economic Prerequisites of Democracy: Reassessing Recent Evidence

*Axel Hadenius and Jan Teorell*

The purpose of this article is to reassess two influential theories of democratic development: the theory of democratic culture and the theory of economic development. The leading predecessors in each domain—Ronald Inglehart and Adam Przeworski—are the prime targets of analysis. We take issue with recent evidence presented by these authors on three grounds: the evidence (1) confuses "basic" criteria of democracy with possible "quality" criteria (Inglehart); (2) conceptualizes democracy in dichotomous rather than continuous terms (Przeworski); and (3) fails to account for endogeneity and contingent effects (Inglehart). In correcting for these shortcomings, we present striking results. In the case of democratic culture, the theory lacks support; neither overt support for democracy nor "self-expression values" affect democratic development. In the case of economic development, earlier findings must be refined. Although the largest impact of modernization is found among more democratized countries, we also find an effect among "semi-democracies."

## Introduction

The emergence of comparative studies of democracy as one of the core empirical fields within political science has been stimulated by two parallel developments: refined conceptualization and measurement, and more advanced research design and methodology. In this article we reassess two influential theories of democratization—the theory of democratic culture and the theory of economic development (which has often been linked to the so-called Modernization School)—in light of these developments.

Ronald Inglehart has in a series of recent publications advocated the cultural theory by displaying an impressive impact of so-called "emancipative"

---

Axel Hadenius is professor of political science at Uppsala University in Sweden. He is the author of *Democracy and Development* (Cambridge University Press, 1992) and *Institutions and Democratic Citizenship* (Oxford University Press, 2001).

Jan Teorell is associate professor of political science at Uppsala University. His articles on intra-party democracy, social capital, and political participation appear in international journals.

*Studies in Comparative International Development*, Winter 2005, Vol. 39, No. 4, pp. 87-106.

or "self-expression values" on democracy. This effect holds even when controlling for a country's level of economic development and years of previous experience with democracy. Moreover, the effect of "self-expression" values is much more pronounced than that of another cultural construct, more often assumed to be of importance: explicit support for democracy (Welzel et al., 2003; Inglehart, 2003; Inglehart and Welzel, 2003). In another set of intriguing results, Adam Przeworski et al. have leveled a blow at the "Modernization School" by showing that economic development has no effect on democratization. Rather, the impact of economic development is to make democracies endure, once established through other means (Przeworski and Limongi, 1997; Przeworski et al., 2000).

We will take issue with these studies regarding both conceptualization and research design. Specifically, we argue that Inglehart and associates conflate issues of democratization in the basic sense with more demanding "qualitative" criteria of democracy. In addition, due to a flawed research design, they fail to distinguish correlation from causation. Przeworski and colleagues, on the other hand, although more rigorous in terms of research design, fail to distinguish between *degrees* of democracy. Improving our empirical tests in these regards yields striking results. In the case of democratic culture, the theory lacks support; in the case of economic development, earlier findings must be seriously refined.

## (Mis)specifying the Dependent Variable: Conceptualizing Democracy

The results—and indeed also the quality—of any explanatory endeavor naturally rest to a high degree on how the subject matter to be explained is specified. We will concentrate on two crucial aspects of this task with regard to the issue of democratization. The first concerns the criteria through which the "democraticness" of political systems should be assessed. The second concerns the issue of whether there is such a thing as degrees of "democraticness," or whether democracy should rather be treated as an all-or-nothing category.

### *Basic vs. Quality Criteria*

The notion of liberal democracy (and its terminological siblings) refers to the existence of certain fundamental democratic rights involving universal suffrage, free and fair elections, and the upholding of a number of political liberties. In Western liberal tradition, the presence of these rights is regarded as the yardstick of democracy (Holden, 1974; Pennock, 1979). These rights should be seen as the *basic* criteria of democracy. In keeping with these basic criteria, all people are given the opportunity to take part and exert influence in the political process, through elections and by other means.

Yet even if these rights (and associated institutions) are available, the democratic process could leave much to be desired. For one thing, actual participation could be disappointing. For another, the responsiveness of the public organs—and hence the influence exerted by the citizens—could be deficient in many respects. In other words, the democratic process might proceed without infringements, but on a low-quality level in terms of popular involvement or responsiveness. Moreover, actual achievements could also be questioned from other normative viewpoints, e.g., the incidence of direct democracy (see, for example, Barber, 1984), the degree of political competition in elections (Hill, 1994), and criteria related to the so-called "deliberative" theory of democracy (see, for example, Elster, 1998). Hence, the basic democratic institutions are in place but their way of operating could be seen as far from ideal. Concepts such as "delegative democracy" (O'Donnell, 1994) and the distinction between "polyarchy" and "democracy" suggested by Dahl (1972) seek to call attention to this discrepancy.

The discussion about these qualitative criteria—on both a normative and an empirical level—is an important part of the democratic research agenda. Two reminders are called for, however. First, while there is today a fairly broad consensus on the basic criteria of democracy, there is considerable confusion and disagreement, involving deep normative divisions, about the specification of democracy's qualitative criteria. Second, even if we could agree upon a set of qualitative standards, it should be observed that these only apply as a complement—that is, in addition—to the basic criteria. In order to fine-tune democracy, the necessary institutions must be present to an essential degree. Hence, it makes no sense to compare democratic qualities, such as levels of participation or some aspects of responsiveness, in democratic and authoritarian states, because such indicators have democratic implications only in states that are mainly democratic (Altman and Pérez-Liñán, 2002).

Authoritarian regimes (such as military or one-party governments) can sometimes exhibit very high scores on popular participation and may sometimes even be responsive vis-à-vis popular demands, but that does not make them more democratic in the liberal sense. Neither can incidences of "deliberation," nor the use of direct modes of decision-making, be said to have democratic merits if not accompanied by key democratic rights. The important point is that poor performance in terms of basic democratic criteria cannot be compensated for with reference to certain "qualities" in other respects. And, by the same token, good performance in the realm of basic criteria should not be disregarded with reference to some unattained qualitative criteria. This is exactly the point where Inglehart and associates err.

### *Political Culture and Democracy*

In an impressive body of publications, Inglehart (e.g, 1988; 1997) has tried to demonstrate that the old verdict of Almond and Verba (1963) holds true: democracy presupposes a democratic culture. In a recent article written with Christian Welzel and Hans-Dieter Klingemann, Inglehart sets up probably the strongest case hitherto presented for this theory (Welzel et al., 2003). Most importantly, the authors have constructed an explanatory variable that certainly seems to make sense from the point of view of the democratic culture theory. The variable in case is an index involving a number of indicators of "emancipative" or "self-expression values"[1] (which could be seen as a central democracy-driving inclination) around the year 1990. The authors demonstrate that this index is strongly connected with a scale of level of democracy in 1999. The scale is based on Freedom House ratings and the Transparency International (TI) index of corruption; the country scores in the two ratings are multiplied into a joint measurement, which is meant to signify "effective democracy." Moreover, Inglehart and Welzel (2003) show that there is no such strong relationship between democracy and another cultural construct, namely overt support for democracy.

Our objection concerns how the dependent variable in these analyses is specified. The approach is rather unconventional, as it comprises both standard indicators of democracy and corruption scores. The aim is to distinguish actually functioning, "effective" democracy from mere "formal" democracy. The crucial factor is respect for the rule of law by elite actors: this factor decides whether or not stated political rights have real clout, of which the degree of corruption can be seen as a proxy.

Sweeping references to the rule of law as a functional correlate to democracy have been made by other authors (e.g., O'Donnell, 2001; Rose and Mishler, 2002). It must be observed, however, that the rule of law means different things in different spheres of social life. In political life it refers to the actual upholding of such rights as freedom of assembly and freedom of the press. And exactly these rights are usually included among the basic democratic criteria, which are observed by the Freedom House and others. To say that such ratings merely deal with the formal existence of these rights is a misunderstanding (whether or not these are proclaimed in some official document is not the point). In short, these ratings account for the upholding of the rule of law in the area of political rights.

The TI index seeks to capture the rate of corruption in connection with international trade and investments. It is seen as a crude indicator of the rule of law in the economic and the administrative spheres.[2] Good performance in this regard could not compensate, however, for poor performance in the political sphere. Singapore is an illustrative example. This state is almost corruption-free as far as economic and administrative issues are concerned. At the same time, political freedoms are thoroughly repressed; in such affairs the legal organs operate as the faithful servants of the government (Chee, 2001). Hence, the favorable TI rating does not make Singapore more democratic. Bolivia illustrates the opposite connection. It has a very low TI score, indicating a high degree of corruption, and yet Bolivia performs remarkably well as far as elections and political rights are concerned: it has a strong and politically independent electoral commission, and essential political freedoms are respected to a quite satisfactory degree (Whitehead, 2001). Accordingly, prevailing corruption in the sphere of business and administration has not made democratic advancement impossible.

The mistake, thus, is to confuse basic and quality criteria. Given the presence of basic democratic rights, it is not unreasonable to maintain, in our mind, that corruption affects the quality of democracy negatively by breeding political "bossism" (in the guise of clientilism or populism) and by diluting state capacity. Hence, among other things, democratic responsiveness is weakened (Putnam, 1993). In line with this reasoning, Bolivia could be seen as a low-quality democracy. But this is another story. Our focus is the development of the basic features of democracy, among which corruption in the realms observed by TI does not belong.

Inglehart may himself agree with this objection. In another article (Inglehart, 2003), the concept "effective democracy" is (silently) exchanged for the conventional Freedom House scores. Although arguably a change for the better, this swift conceptual redressing casts some doubts on the theoretical stringency of his research enterprise.

### *Dichotomous vs. Continuous Measures*

This brings us to the question of dichotomous versus continuous measures of democracy. First and foremost we concur with Collier and Adcock (1999) that any effort to choose between these two strategies by reference to the "inherent" or "essential" nature of democracy appears to be a dead end. In common parlance we use the word "democracy" in both ways: sometimes to make the discrete claim that some countries are "democracies" whereas others are not, and sometimes to make the graded statement that some countries are more "democratic" than others. However, the fact that democracy in general could be conceived of in both dichotomous and continuous terms does not imply that either approach is always equally justifiable. We argue instead that when democracy is to be analyzed as a key empirical property of political systems—be it as a dependent or independent variable—there are strong reasons to prefer the continuous approach. The *prima facie* argument is substantive: once the view that democracy *can* be treated in continuous terms is accepted, merely a quick glance at the world around us implies that dichotomous classifications incur a considerable loss of information. If one accepts the claim that with regard to democratic standards, Singapore performs better than Burma, and that conditions in turn are far better in Bolivia, and better still in Costa Rica, why should this information be wasted by collapsing the countries into "autocracies" and "democracies," disregarding the graded differences and intermediate cases?

There is also a methodological reason to prefer a continuous approach—simply that it is more reliable than a dichotomous measure. Measurement imprecision must be accepted as a basic given when it comes to such a complex concept as democracy. The effects of erroneous classifications, however, are generally more serious when only two categories are used, as compared to continuous multi-category scales (Bollen, 1990: 13). We will now discuss the findings of Przeworski and associates in light of these two arguments against the dichotomous approach.

### *Economic Development and Democracy*

Przeworski and colleagues contribute to a long-standing debate, dating back at least to Seymour Lipset's (1959) claim that economic development promotes democracy. According to the original theory—the so-called Modernization School—economic development is held to be a causal factor, effecting the move from authoritarianism to democracy (Diamond, 1992; Burkhart and Lewis-Beck, 1994). In challenging this theoretical conception, Przeworski et al. (2000) make the argument that development does not drive the process of democratization, but plays a role when this process is accomplished successfully; it upholds democracy once it is in place. Hence, the process of modernization does not, as the original theory predicted, *generate* democracy, but it does contribute greatly to the potential for regime sustainability in already democratic states. Consequently, it is possible to make fairly reliable forecasts (based on GDP levels) concerning democratic sustainability, while the rise of democracy remains a "black box" as far as such structural conditions go. These results—supported by massive empirical data that are processed by advanced statistical techniques—have sent a strong message to the research community.[3]

The results have not gone unchallenged, however. In a recent study, Boix and Stokes (2003) make two points of criticism. The first—which was also made by Coppedge (2003: 124)—is that Przeworski et al. (2000) misinterpret their own results. In their more fully specified model of regime dynamics (presented in Table 2.17, p. 124), economic development actually does affect the probability of a transition to democracy, although the effect on democratic sustainability is far more substantial. Second, Boix and Stokes show that when the analysis is extended back in history, different results ensue. In the period from 1850 to 1950, economic development appears to have a larger impact on transitions to democracy than on democratic sustainability.[4]

Much like Przeworski and associates, however, Boix and Stokes base their conclusions on a dichotomous measure of democracy. Countries are classified as being democratic or not, based on the incidence of elected officials, in combination with a judgment on the way the elections are held (Przeworski et al., 2000: 14-30; Boix and Stokes, 2003: 529). These, of course, are very limited criteria, focusing only on elections and disregarding possible violations of various political liberties. Moreover, no special attention is paid to the fairness of the electoral process with regard to suffrage, ballot rigging, or the banning (or discrimination) of certain parties. In other words, neither Przeworski and associates nor Boix and Stokes can be criticized for failing to concentrate on the *basic* criteria of democracy. Rather, they can be criticized for being too minimalist, by excluding some theoretically relevant aspects of democracy from their measurement.

Of more crucial importance, however, is the dichotomous approach itself. As for the choice of this approach, Przewroski and colleagues simply maintain that democracy *is* an either/or phenomenon. It makes no sense, in their judgment, to talk about half-democracies. Democracy is present or not—there is "a natural zero point" (2000: 57). However, this argument is no more convincing than any counterargument based on claims about the "inherent continuous nature" of democracy (Bollen and Jackman, 1986: 16). They all border on conceptual reification.

The second reason to opt for the continuous approach, stemming from reliability considerations, might at first come as a surprise since Przeworski et al. themselves make the claim that under reasonable distributional assumptions, "there is less measurement error when a dichotomous scale is used" (2000: 59). However, as Elkins (2000: 298-299) has convincingly shown, this claim stems from a flawed measure of reliability. More specifically, Przeworski and associates only evaluate the *error variance* of two hypothetical measures, whereas the correct yardstick is to compare the *ratio* of the error variance to the *total* variance. According to this criterion, Elkins shows, the continuous measurement strategy fares much better. Only under extreme circumstances would a dichotomous measure outperform a continuous one in terms of reliability.

In sum, the focus on electoral democracy, in a limited sense, and the use of a dichotomy (which is not a happy combination) raise doubts about the accuracy of both the study conducted by Przeworski and his associates and of their main contenders in the literature. We shall now see if their findings hold up when other measurements and another research design are applied.

## Issues of Research Design: Dynamic Models of Democratization

Political scientists of all inclinations are aware of the dictum "correlation does not prove causation." This is generally taken to imply that bivariate analysis of the relationship between *X* and *Y* is not enough. If other possibly confounding variables, such as *Z*, are not controlled for, the estimated effect of *X* on *Y* is biased. Perhaps less well known, however, is the fact that even a well-specified model—in the sense of having all relevant controls included—might yield grossly erroneous results. There are two reasons why this might be the case: (a) there is a problem of endogeneity, i.e., there might be a causal arrow running from *Y* to *X*; or (b) the causal lag, i.e., the time it takes for *X* to affect *Y*, is not taken into account.

With cross-sectional data, not much can be done about these problems. Without strong modeling assumptions that are rarely met in practice, endogeneity cannot be controlled for. Moreover, all variables must be measured at the same time point, leaving no room for causal lags. When the time-series dimension is added, however, the data can provide some leverage on both problems. More specifically, the explanatory variable *X* may be "lagged" in order to achieve the proper length of time it takes to affect *Y* at time *t*. Thus, if the proper lag length is known to be *q*, *X* at *t-q* is added to the equation instead of *X* at *t*. Moreover, by also lagging the dependent variable—that is, by including *Y* at *t-q* on the right-hand side of the equation—the model assures that any effects of *Y* on *X* occurring previous to *t-q* are controlled for. Most importantly, the inclusion of *Y* at *t-q* rules out any effects due to the path $Yt \leftarrow Yt\text{-}q \leftarrow Yt\text{-}2q \rightarrow Xt\text{-}q$. In this way, given that *q* is properly specified, the problems of endogeneity and time lags are handled (see, for example, Finkel, 1995: 24-31).

In the recent articles by Inglehart and associates (Welzel et al., 2003; Inglehart and Welzel, 2003), only one of these design strategies is taken into consideration: specification of the lag length. The basic setup is to regress democracy at the latest time point possible (which appears to have been 1999) on cultural values at the earliest time point possible (about 1990). Although theoretical expectations as to lag length are never discussed, the implicit assumption appears to be that it takes time for culture to affect democracy. Thus, the longest lag length permissible by data (which happens to be around nine years) is used.

This is certainly an improvement over earlier static, cross-sectional tests of the cultural thesis (see, for example, Inglehart, 1988). Yet the possibility that democratic institutions are not the effect but the *cause* of political culture is still not ruled out, despite the authors' claim to the contrary.[5] This flaw is noteworthy, considering the recurrent claim that the cultural theory has the dynamics backward in exactly this way (Barry, 1970; Rustow, 1970; Muller and Seligson, 1994). Precisely for this reason we will in the analyses that follow control for this possible causal direction by including the lagged dependent variable.

Nevertheless, we do acknowledge one seldom-recognized drawback with the lagged dependent variable approach, if used alone (however, see Inglehart, 1997: 199-202). The approach treats all countries that do not change as a uniform entity—they are implicitly assumed to have the same explanatory background. However, the explanation for immobility for the countries at the top of the democratic scale may differ from that of the countries at the bottom. Political stability in Norway and North Korea, or in Canada and Cameroon, are not, in all probability, due to the same conditions. Likewise, changes in countries' positions on the scale could very well depend on different factors at different levels of democracy. Accordingly, we need a statistical model that can capture the extent to which explanatory variables have different effects in more or less democratic contexts. In effect, this is exactly the requirement we need to fulfill if we are to test Przeworski and associates' thesis on the two dynamics of democratization. The most straightforward way to accomplish this is to include an interaction term as an additional control variable.[6] Thus, stated in equation form, the simplest version of our statistical model is

$$Y_{i,t} = b_0 + b_1 \cdot X_{i,t-q} + b_2 \cdot Y_{i,t-q} + b_3 \cdot X_{i,t-q} \cdot Y_{i,t-q} + e_{i,t}$$

where $Y$ is the level of democracy in country $i$ at time $t$, $X$ some explanatory variable, $q$ the lag length, and $e$ the error term. If the coefficient for the interaction term ($b_3$) is statistically significant, there is context dependence in the impact of the corresponding causal variable.

A final note on this model is the question of causal lags. What $q$ should be used in the estimation set-up? It is a well-known fact among time-series and panel methodologists that this is an extremely difficult question to answer on the basis of empirical data (Griliches, 1967; Finkel, 1995: 13). We shall make no such attempt in this article. Instead, we will simply use the same lags as our predecessors. In the case of Inglehart and associates (2003), this means a lag of approximately nine years. With respect to Przeworski et al., on the other hand, the causal lag is assumed to be only one year.

## Data and Measures

In the empirical analyses that follow, we use the following data and measures:

*Democracy*. Our data on the fulfillment of the basic criteria democracy derive from Freedom House. We convert the combined 1–7 rankings of civil and political liberties into a democracy index running from 0 (complete autocracy) to 100 (basic criteria of democracy fulfilled). This data set is known to suffer from certain weaknesses. The methods of coding are not fully transparent, and one could question the validity of some of the classification criteria in the case of civil liberties; it could be argued that too much is included (Munck and Verkuilen, 2002). Furthermore, some researchers have pointed to a geographical-cum-political bias, as some regions and regimes are treated more favorably in Freedom House than in other ratings.[7] On the positive side, the Freedom House data stretch over a relatively long period of time, which makes time-series analyses possible. Besides, the overall validity of the criteria applied must be judged as relatively satisfactory, as the index is composed of a broad set of (theoretically relevant) electoral and civil rights. These two sets of rights are certainly closely correlated in many cases (Hadenius, 1992). Nevertheless, important differences in performance in these regards do occur. If one looks, for instance, only at the electoral aspect, one risks ending up with the "electoral fallacy" that conflates a constrained electoral democracy with fully-fledged liberal democracy (Karl, 1990; Diamond, 2002). As for the length of the time period, even more appealing alternatives appear to be available, such as the Polity data. However, the Polity data are composed on the basis of criteria related to elections and institutional divisions of power—but with no reference to civil liberties. Besides, the aggregate scores are strongly affected by one criterion in particular: constraints on the chief executive, which could hardly be seen as the most central democracy indicator (Gleditsch and Ward, 1997). These deficiencies make this data set less attractive.[8]

*"Effective Democracy."* In order to replicate the results of Inglehart and associates (2003), we also compute their index of "effective democracy." This index is computed by simply multiplying our Freedom House–based democracy rating with the Corruption Perception Index of Transparency International for the same year, and then converting the result back to a 0–100 scale.

*Self-Expression Values*. Since our aim is to replicate the findings of Inglehart and associates as closely as possible, we have tried to follow their technique for constructing the primary cultural construct "self-expression values." This is an index based on factor scores from a principal component analysis of a number of items of the World Value Surveys (WVS) performed at the aggregate level. The items tap four aspects of political culture: tolerance of diversity, inclination to civic protest, liberty aspirations, social trust,and life satisfaction. The time of measurement is the year of the second wave of the WVS, or about 1990. For countries that were not included in the second wave, data from the third wave have been used to impute values of the index.

Whereas Inglehart and associates have access to the fourth wave of the WVS conducted around 2000, leaving them with 73 countries in all, we have to rely on the smaller subset of countries available in the published World Value Surveys database (Inglehart et al., 2000). The published data set includes 87 surveys from 61 countries, with 41 surveyed in 1990 and 46 in 1995 (26 countries were thus surveyed at both time points).[9] This means that some countries that had not been part of the WVS before 2000, or that for other reasons are not part of the published file, are missing from our data set.[10] Another implication is that in 11 country-surveys, we must rely on data in which some of the indicators making up the index of self-expression values are missing. Data for these indicators have been imputed by the predicted values from a regression on all other indicators in the index.[11] All in all, this leaves us with data on self-expression values for 60 countries around 1990.

*Support for Democracy*. The variable tapping support for democracy is the percentage of respondents within each country scoring at least 4 or more on a –6 (maximum autocratic support) to +6 (maximum democratic support) additive index of the four items on support for democracy. The items tap disapproval of military rule and the rule of strong leaders, and approval of democracy. Inglehart and Welzel (2003) term this index the "Percentage of Solid Democrats," since "from +4 onward you are closer to the maximum democratic support (+6) than to the neutral point (0)." The items making up this index were not included until the second wave of the WVS, which results in data on fewer countries. Moreover, again due to our restricted data set, we are missing values on support for democracy in five East European countries included Inglehart and associates' data set,[12] leaving us with a total of 41 countries.

*Economic Development*. This variable is measured as logged GDP per capita at purchasing power parity. The economic data cover 155 countries in the period from 1975 to 2001, and are published by the World Bank as part of the *World Development Indicators*.

## Does Political Culture Have an Effect on Democracy?

As both model A.1 in the upper panel and model B.1 in the lower panel of Table 1 indicate, there is a close match between our findings and those of Inglehart and associates. Both the self-expression values and support for democracy indices yield coefficients very similar to our predecessors. Thus, we can proceed with some confidence that the results that follow are not an artifact of our more limited data set.

The first step towards a more appropriate test of causal relationships, then, is to substitute the debatable construct "effective democracy" as the dependent variable by our democracy rating. This is what happens in model 2 for each panel.[13] Let us first concentrate on the effect of self-expression values, reported in the upper panel. As can be seen, its impact decreases some 40 percent when the *Corruption Perceptions* are partialed out from the dependent variable. The corresponding explained variance drops by half. Thus, already by avoiding the confusion of basic criteria of democracy with the incidence of corruption, the case for self-expression values is weakened considerably. Nevertheless, their estimated impact remains statistically significant. By turning to model A.3, however, we exclude the possibility that this relationship is due to backward causation from democracy to self-expression values at earlier time points in history. Once this possibility has been ruled out, nothing remains of the effect of self-expression values. Its causal impact, when properly estimated, is minuscule and insignificant. Adding an interaction term to this dwarfed im-

## Table 1
## The Effect of Political Culture on Democracy

PANEL A

| *Dependent Variable:* | Effective democracy in 1999 | | Democracy in 1999 | | |
|---|---|---|---|---|---|
| *Independent Variables:* | *I & W* | (A.1) | (A.2) | (A.3) | (A.4) |
| Self-Expression Values in 1990 | *27.15**** | 25.06*** | 14.81*** | 4.06 | –4.02 |
| | *(1.94)* | (2.09) | (2.54) | (3.39) | (12.4) |
| | *β=.86* | β=.85 | β=.61 | β=.17 | β=–.16 |
| Democracy in 1990 | — | — | — | .518*** | .576*** |
| | | | | (.123) | (.151) |
| | | | | β=.59 | β=.65 |
| Self-Expression Values *Democracy in 1990 | — | — | — | — | .093 |
| | | | | | (.138) |
| | | | | | β=.29 |
| Constant | *43.61**** | 49.01*** | 79.62*** | 41.24*** | 34.98*** |
| Adjusted $R^2$ | *0.74* | 0.72 | 0.36 | 0.50 | 0.49 |
| N | *68* | 57 | 60 | 60 | 60 |

PANEL B

| *Dependent Variable:* | Effective democracy in 1999 | | Democracy in 1999 | | |
|---|---|---|---|---|---|
| *Independent Variables:* | *I & W* | (B.1) | (B.2) | (B.3) | (B.4) |
| Support for Democracy in 1995 | *.91**** | .878*** | .361* | .073 | –.004 |
| | *(.19)* | (.209) | (.181) | (.094) | (.345) |
| | *β=.60* | β=.57 | β=.30 | β=.06 | β=–.04 |
| Democracy in 1995 | — | — | — | .741*** | .691*** |
| | | | | (.068) | (.226) |
| | | | | β=.86 | β=.81 |
| Support for Democracy *Democracy in 1995 | — | — | — | — | .001 |
| | | | | | (.005) |
| | | | | | β=.10 |
| Constant | *–2.25* | 4.56 | 57.1*** | 19.0*** | 22.3 |
| Adjusted $R^2$ | *0.34* | 0.31 | 0.07 | 0.77 | 0.76 |
| N | *44* | 38 | 41 | 41 | 41 |

* Significant at .10 level.
** Significant at .05 level.
*** Significant at .01 level.

Note: *Entries* in the remaining columns are unstandardized regression coefficients (β denoting the standardized coefficients), with standard errors in parentheses.

Italicized entries in the column marked *I and W* are reproduced from Table 2, Models 1 and 4, in Inglehart and Welzel (2003: 71).

pact, as in model A.4, does not change the gloomy picture. It is not the case that contextual dependence conceals a real effect of self-expression values on democracy. There simply is no effect to conceal.

On the other hand, this holds true of support for democracy, as well. As model B.2 shows, in this case there is hardly any effect on democracy even before making the correct dynamic specification. That model B.3-5 shows that support for democracy has no impact under proper test conditions should not, therefore, come as a surprise.

Thus, in this latter sense the sturdy claims by Welzel et al. (2003), Inglehart and Welzel (2003), and Inglehart (2003) hold water: public support for democracy does not have a causal impact on democracy. More importantly, however—and in sharp contrast to their view—we have shown that *neither do self-expression values*.[14] The evidence Inglehart and colleagues present to substantiate their claim to the contrary is boosted by two factors: first, they confuse the level of democracy with the level of corruption,[15] and second, they fail to control for the lagged dependent variable.

With regard to the latter, it should be noted that this is not the first time Inglehart's findings have been shown to vanish with the introduction of a lagged dependent variable (Muller and Seligson, 1994). In response to this criticism, Inglehart (1997: 202) claimed that the lagged dependent variable approach "does not address the question of whether cultural factors are responsible for the long-term survival or failure of democracy." Instead, it addresses the question of whether political culture is conducive to shifts in levels of democracy observed between particular years. Thus, it creates an analysis that is "clearly the wrong dependent variable to test the role of culture." We disagree. The appropriate test for a *causal* relationship is to study shifts between those years that are amenable to analysis, and they happen to be around 1990 and 1999.

Culturalists might argue that this longest possible lag is still too short.[16] Perhaps the causal process is so slow that it would take 50 or even 100 years before it could be observed properly. Yet in the meantime, we will have to rely on the causal lags that the present data allow. These might be too short, but that is not an argument for not testing them. And if they are put to test, as it now stands political culture does not seem to have a causal impact on democracy. Still, culturalists could hope for future data to verify their stance.

## Economic Development and the Dynamics of Democratization

We shall now change track and turn our attention to the second theoretical contender, that is, the argument by Przeworski et al. (2000) that the effect of economic development on democracy differs between authoritarian and democratic countries. To our knowledge, this is the first time this argument has been applied to data covering the 1990s and, more importantly, to the more fine-grained measure of democracy that the Freedom House ratings represent. Table 2 presents the results.[17]

In model 1, we simply regress democracy at time $t$ on the level of economic development, measured as logged GDP per capita at purchasing power parity, at time $t$-1 (that is, one year earlier). That we find a statistically significant positive effect should come as no surprise.[18] But whereas this first model specification establishes the correct time order between the independent and dependent variables, it does not control for the possibility that democracy at earlier time points may have generated higher levels of economic development. This possibility is ruled out by model 2. When democracy at time *t*-1 is controlled for, economic development looses an incredible amount of its short-term impact. Nevertheless, the fact that it remains statistically significant, despite facing such a ferocious rival as the lagged dependent variable, lends strong support to the modernization thesis. Moreover, the long-term effect (the so-called "long-run multiplier") is $0.642/(1-0.955)\approx 14.27$—not far from the cross-sectional estimate of model 1. In sum, a high level of economic development is conducive to higher levels of democracy.

But then again, this is exactly what we should expect in light of some forty years of empirical analysis on the subject. The crucial test of the modernization thesis is the one performed by model 3. Here we add an interaction term, designed to pick up the degree to which the effect of economic development is contingent on a country's previous level of democracy. This interaction term turns out to be positive and statistically significant,[19] indicating that the higher the level of democracy at a certain time point, the larger the effect of economic development on the future prospects for democracy. Thus, it is not a universal truth that economic development fosters democratic advancement. The effect is dependent on context.

More specifically, the effect starts from a low level in non-democratic countries. The negative but insignificant coefficient for logged GDP (–0.166) means that the effect of economic development in completely authoritarian countries (receiving the score of 0 on our democracy rating) is close to zero. However,

**Table 2**
**The Dynamic Effect of Economic Development on Democracy**

| | *Regression models:* | | |
|---|---|---|---|
| *Variables:* | (1) | (2) | (3) |
| Logged GDP per capita at *t*-1 | 17.8*** | .642** | –0.166 |
| | (.511) | (.276) | (.326) |
| Democracy at *t*-1 | — | .955*** | .832*** |
| | | (.012) | (.049) |
| Logged GDP*Democracy at *t*-1 | — | — | .015*** |
| | | | (.005) |
| Constant | –88.0*** | –2.17 | 4.12* |
| Adjusted $R^2$ | 0.38 | 0.94 | 0.94 |

* Significant at .10 level.
** Significant at .05 level.
*** Significant at .01 level.

Note: Entries are unstandardized regression coefficients with panel corrected standard errors in parentheses; N = 3382, with data from 155 countries in 1975-2001, or 22 years per country on average.

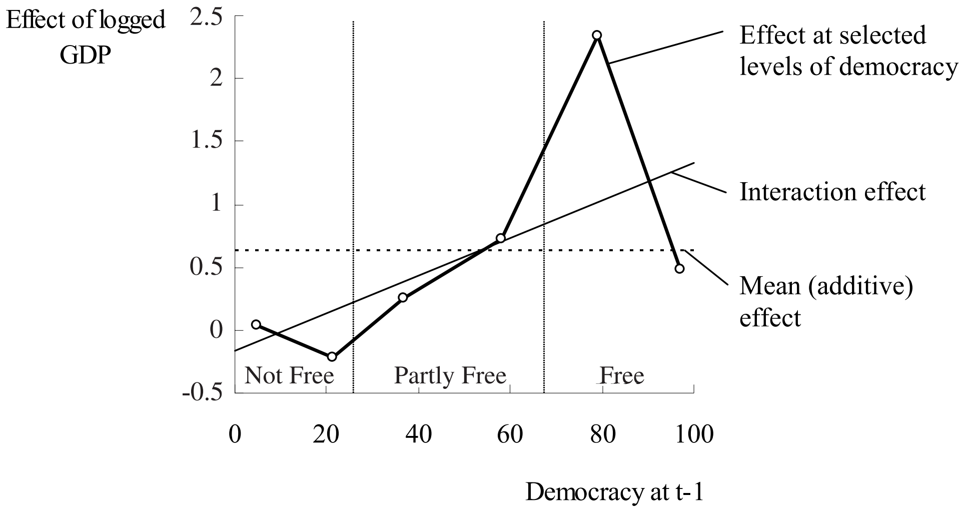
the effect increases as countries—presumably propelled by other factors—move toward democracy. At the other end of the scale, in countries that fulfill all the basic standards of democracy (that is, the score 100), the effect is more substantial. It can easily be calculated by multiplying the effect of the interaction term (0.015) by 100 and add this product to –0.166, which yields an estimated effect of 1.33. Since countries at this end of the scale cannot move further upwards, this effect must be interpreted as a force working against authoritarian backsliding. In other words, the less economically developed a country, the further it will move away from democracy and toward authoritarian rule.[20]

Thus, our findings parallel those of Przeworski and associates. The dynamics of economic development work differently in more democratic as compared to more authoritarian contexts. In the perfectly authoritarian setting transitions toward democracy are *not* more likely to occur as a result of economic prosperity. Once the basic institutions of democracy have been installed to higher degree, however, transitions toward authoritarianism are less likely to occur in more economically developed countries.

But the fact that we measure democracy on a semi-continuous scale, and not as a dichotomous entity, enables us to take the test one step beyond Przeworski and colleagues. Model 3 assumes a linear relationship between democracy at time *t*-1 and the effect of economic development. One way to relax that assumption is to proceed in a more step-wise manner, and estimate the effect at different selected levels of democratic development. We report the results from such a procedure in Figure 1.[21] The vertical axis plots the effect of logged GDP per capita given the score of our democracy rating at time *t*-1 on the horizontal axis. The dashed and thin lines simply reproduce the results from models 2 and 3 in Table 2. That is, if we assume that economic development has a similar effect in all countries, its estimated impact is on average approximately 0.642 (the dashed horizontal line). But this conceals the important fact that as we move from perfectly authoritarian countries to the opposite end of the democracy scale, the effect of economic development rises from just below zero (–0.166) to somewhere around 1.33 (the thin slope). Yet the circled points connected by a thick line tells us that this, too, is not the whole story. The effect of economic development has a markedly different effect at different levels of democracy, but this relationship cannot be fully captured by a linear function.

To ease interpretation, we have indicated the three zones of democracy that Freedom House customarily reports: countries that are “free,” “partly free,” and “not free.” Incidentally, the distinction between “free” and “partly free” countries roughly corresponds to the average cutting line between democracies and autocracies applied by Przeworski and associates.[22] Thus, if their results were to be perfectly reproduced in our data, we should only find an effect of economic development within the “free” zone. But this is not the case. The effect of economic development is present already among countries that are “partly free.” This is particularly the case for the group of countries whose democracy ratings lie in the range of 50 to 66.7 (corresponding to a “raw” mean Freedom House rating of 3–4 on the 1–7 scale). Among these countries

**Figure 1**
**Contingent Effects of Economic Development on Democracy**



the effect of economic development roughly corresponds to the mean effect among all countries. This is the first point where the argument of Przeworski and associates needs to be refined. In their dichotomous world-view, economic development only has an effect among already "established democracies" (countries classified as "free"). The more fine-grained message is that the effect of economic development kicks in before that threshold.

The second point worth noting is that the effect is substantially different *among* the "established democracies." As is shown in Figure 1, the effect peaks among the countries with a democracy rating around 80, that is, just above the threshold separating "partly free" from "free" countries (corresponding to a "raw" mean Freedom House rating of 2–2.5). It then dives steeply, even falling below the mean effect, among the countries at the very end of the democracy rating. We can make no conclusive interpretation of this downturn. The most plausible scenario, however, would be that it represents a ceiling effect. At this end of the scale, as mentioned earlier, only moves toward authoritarianism are possible. Among these countries economic development can only affect backsliding. In countries around 80 on the scale, on the other hand, moves are possible both in an upwards and downwards direction. This might explain why economic development here appears to have a larger impact.

## Concluding Remarks

The aim of this article has been to probe the empirical tenability of two prominent theories of democratization: one focused on democratic culture, the other on economic development. According to the cultural theory, a key prerequisite of democracy is support for democratic norms (and associated behavior) among the citizenry at large (Almond and Verba, 1963). Therefore, tapping the opinion among the general public about beliefs in democracy is the appropriate method of gaining insight into the prospects of democratic government. As is well known, such investigations have become almost an industry. The world is full of "value surveys" and "barometers" which register popular attitudes to democracy (and pertaining issues) on a regular basis. Ronald Inglehart is currently the leading personality of this enterprise, both as research entrepreneur and analyst.

The usual way of going about these surveys is to pose questions regarding support for democracy—both the principles of democracy and its alternatives. According to several studies in the field, the degree of mass support for democracy could be taken as an indicator of democratic rootedness (or consolidation). Broad popular support for democracy is seen as an indicator of it being the "only game in town" (Diamond, 1999; Bratton and Mattes, 2001; Schedler, 2001). In critique of this conception—which has mainly been taken for granted—Inglehart and his associates have argued in recent articles that direct questions about democratic support give but a shallow depiction of the attitudinal landscape in question. Overt support of democracy is mainly "lip service," as it is generally high in democracies as well as in authoritarian states. This argument hinges on an assumed system effect: given some form of causal relationship between culture and system, support ought to be considerably lower in undemocratic countries. Thus, something must be wrong with the indicator. The point is substantiated by the fact that another cultural trait, self-expression values, gives a far better match. This illustrates, according to Inglehart and his associates, that the variable captures a more deeply held democratic inclination (Inglehart, 2003; Inglehart and Welzel, 2003).

As we have illustrated in this article, Inglehart's claim does not hold. Probing for the democratic effects, none of these cultural traits seem to have an impact. Using a more appropriate dependent variable and a research design that accounts for changes over time and for interactive effects, neither the self-expression index nor the democracy-index stand up to control for economic development. Hence, the cultural thesis—in both versions—is rejected. The degree of popular support for democracy does not seem to be a reliable predictor of democratic development.

The same cannot be said about economic theses, however. Having criticized Przeworski and his colleagues for their way of dealing with democracy as an either/or phenomenon, and having applied a graded measurement instead, we come to the conclusion that—as stated by our predecessors—economic development does have a stronger impact among states that are more fully democratic. Moreover, economic development does not appear to be "the cure" for states at the bottom of the democracy ladder. At this stage economic improvements seem to bear no democratic fruits. But this is not the full story. By using a graded measure of democracy we can demonstrate that a significant (and increasing) effect emerges already among "semi-democracies." This finding has important theoretical repercussions. It lends support to the original understanding of the process of modernization, which holds that economic development has a genetic effect on democracy. As we know, this understanding has been opposed by Przeworski et al. Our inquiry does not substantiate that objection. It shows that the coming of democracy is *not* a "black box"; it is not a non-structural outcome, as it has often been held in recent surveys of the research literature (see, e.g. Carothers, 2002).

To sum up, Przeworski and associates are right and wrong. They are right about the absence of a modernization effect among autocracies—that is, countries classified as "not free." In these countries economic development does not generate democracy. However, they are wrong about the stage of democratic development where the modernization effect sets in. The effect occurs below the cut point where they distinguish democracies from autocracies.

## Notes

1. Welzel et al. (2003) use the term "emancipative," whereas Inglehart and Welzel (2003) use the term "self-expression" values, although the two cultural constructs are measured almost identically. We have opted for the latter term (and construct) in this article.
2. In the administrative realm the rule of law refers to such characteristics as transparency and accountability (in an auditing sense) and to the upholding of procedural regulations. In the economic realm it refers to respect for property rights and equal treatment in business relationships (Goldsworthy, 2001). The custom of paying of bribes usually infringes on the rule of law in both reams.
3. The findings regarding the "non-structural" character of the process of democratic transition link up with the influential study on this theme by O'Donnell and Schmitter (1986). According to these authors, the introduction of democracy is mainly agency-driven. As for the impact of work by Przeworski et al., see, for instance, the review by Coppedge, who rates it "one of the most important books in comparative politics in the last decade" (2003: 123).
4. Gasiorowski (1995), also working with a dichotomous democracy variable, actually came up with this result in his sample of large Third World countries from the 1950s throughout the 1980s. In his model economic development had a significant impact on transitions to democracy, but not on transitions to autocracy (1995: 889, Table 2).
5. Welzel et al. (2003: 32-33) and Inglehart and Welzel (2003: 70-71) claim that they exclude this reverse causal relationship by controlling for "prior democratic traditions." This control variable is a count of the number of years a country has received a score of 7 or higher on the "Combined Polity Scale" since independence or 1850 (whichever came first). However, this is another theoretical construct than the dependent variable in question, that is, the level of democracy. Thus, although a step in the right direction, this control does not preclude confounding effects of endogeneity.
6. Thus, it should come as no surprise that Przeworski and colleagues model regime type R at time $t$ as a function of an exogenous variable X at time $t$-1 and the interaction effect of X and R at $t$-1 (2000: 139). However, by omitting the regime type $R$ at time $t$-1 on the right-hand side of the equation, their model forces the intercept to be the same for democracies and dictatorships. This could lead to misspecification error, possibly dampening the effect of economic development among dictatorships.
7. Bollen (1993) and Bollen and Paxton (2000). It should be observed, however, that these evaluations suffer from considerable insufficiencies: none of the ratings that are used as points of reference (Banks, 1979; Sussman, 1982) can be regarded as theoretically accurate and unbiased sources of evidence.
8. Yet for comparative purposes, results based on Polity data will be examined as well. See footnote 19 below.
9. We have tried to replicate as closely as possible the way Inglehart and associates treat countries. In two respects, however, we deviate from their procedures: in Colombia only the survey conducted in 1997 is included, and East and West Germany are treated as one country.
10. The new WVS countries in 2000 are Egypt, El Salvador, Greece, Iran, Jordan, Luxembourg, Malta, Uganda, and Zimbabwe. Moreover, Albania and New Zealand conducted their surveys in the second wave too late to be included in the data file.

11. In the case of Pakistan, where three out of five indicators were missing, we refrained from imputation. Thus, Pakistan is missing in our analyses of the self-expression dimension.
12. These are Albania, Czech Republic, Slovakia, Hungary, and Romania.
13. It should be noted that we then gain three countries which are missing on the Corruption Perception index in 1999: Bangladesh, the Dominican Republic, and Bosnia. However, in neither panel are the results dependent on this gain (i.e., we get the same results if these countries are excluded).
14. These findings also hold if measurement error in the cultural constructs are taken into account. We have estimated a structural equation version of models A.3 and B.3 in Table 1, using aggregate means of the relevant items as observed indicators of the latent variables self-expression values and support for democracy. Neither latent construct has a statistically significant effect (even at the p= .10 level) on democracy in 1999, after controlling for democracy in 1990 or 1995, respectively. The models were estimated using LISREL 8.54.
15. That this boost is independent of the second can be shown by controlling model A.2 for "effective democracy" in 1995 (the earliest time point available on the Corruption Perception-index); there is then no longer a causal impact of self-expression values on "effective democracy."
16. It should be noted that the effect of both self-expression values and support for democracy remain trivial and insignificant even if the latest Freedom House scores, depicting democracy in the world in 2002, are used to construct the dependent variable.
17. To account for contemporaneous spatial autocorrelation and panel heteroskedasticity, we present the "panel corrected standard errors" recommended by Beck and Katz (1995; 1996). Lagrange multiplier tests (Beck and Katz 1996: 9) indicate no presence of serial autocorrelation once the lagged dependent variable has been introduced: the autocorrelation coefficient is miniscule (0,04 in model 2, 0.05 in model 3) and non-significant.
18. It should be noted that our estimate is almost identical to what Helliwell (1994: Table 1, Eq. 1) found for a much more restricted sample of 125 countries from 1976 to 1985. Helliwell's corresponding estimate is 20.5. This is strong evidence as to the robustness of the economic development effect.
19. We find a positive and significant interaction effect under a number of different specifications. Increasing the lag length to five or ten years only strengthens our results. Changing the independent variable to (lagged and logged) energy consumption per capita—according to Burkhart and Lewis-Beck "the economic development measure most often used" (1994: 905)—yields very similar results. Adding control variables, such as British colonial history, the proportion of Protestants, Catholics or Moslems, religious fractionalization, and per capita growth, also does not affect our findings. Finally, changing the dependent variable to the democracy rating presented in Polity IV leads to similar results in terms of coefficient magnitudes, but the interaction effect is now just below the threshold for statistical significance (p= 0.11). However, this might be due to a loss of observations (the Polity IV-data cover some 10 fewer countries in the observation period 1975-2001).
20. The strong coefficient of the autoregressive term (especially in model 2) may indicate that the time-series is, if not integrated, so at least "near-integrated" (De Boef and Granato, 1997, 1999). We have thus followed the methodological advice to reestimate all results using the error correction model (Beck and Katz, 1996: 10-12). Focusing on the long-term impact of economic development, this alternative specification yields almost identical results.
21. To estimate the effect of economic development at different levels of democracy, we have simply divided our sample of 3,382 country years into thirteen groups, corresponding to the thirteen different values possible on the democracy rating. Model 1 in Table 5 is then estimated for each of these groups, generating thirteen estimated regression coefficients for the effect of logged GDP. Since some of the groups have a rather small number of cases, however, we have collapsed them into six larger groups in order to generate more robust results. The ranges of the democracy rating among these six groups are: (1) 0-8.33; (2) 16.7-25; (3) 33.3-41.7; (4) 50-66.7; (5) 75-83.3; and (6) 91.7-100. The plotted circles connected by a thick line in Figure 1 are the 6 weighted means of the corresponding 13 estimates.
22. This cutting-line has been estimated in two ways. First, we distinguished all country years from 1972 to 1990 (the time-period where these two data sets overlap) where transitions just had occurred from autocracy to democracy according to the Przeworski-dichotomy, and estimated the mean value of our Freedom House–based democracy rating among these cases. Second, we distinguished all country years where transitions from democracy to autocracy were just about to take place. The corresponding mean yield similar results.

## References

Almond, Gabriel and Sidney Verba. 1963. *The Civic Culture*. Princeton: Princeton University Press.

Altman, David and Aníbal Pérez-Liñán. 2002. "Assessing the Quality of Democracy: Freedom, Competitiveness, and Participation in Eighteen Latin American Countries." *Democratization* 9: 85-100.

Banks, Kenneth. 1979. *Cross-National Time-Series Data Archive User's Manual.* Binghamton: State University of New York.

Barber, Benjamin. 1984. *Strong Democracy: Participatory Politics for a New Age*. Berkeley: University of California Press.

Barry, Brian. 1970. *Sociologists, Economists, and Democracy*. London: Macmillan.

Beck, Nathaniel and Jonathan Katz. 1995. "What to Do (and Not to Do) with Time-Series Cross-Section data." *American Political Science Review* 89, 3: 634-647.

_______. 1996. "Nuisance vs. Substance: Specifying and Estimating Time-Series Cross-Section Models." *Political Analysis* 6: 1-36.

Boix, Carles and Susan Stokes. 2003. "Endogenous Democratization," *World Politics* 55 (July): 815-849.

Bollen, Kenneth. 1990. "Political Democracy: Conceptual and Measurement Traps." *Studies in Comparative International Development* 25, 1: 7-24.

_______. 1993. "Liberal Democracy: Validity and Method Factors in Cross-National Measures." *American Journal of Political Science* 37, 4: 1207-1230.

Bollen, Kenneth and Robert Jackman. 1989. "Democracy, Stability, and Dichotomies." *American Sociological Review* 54: 612-621.

Bollen, Kenneth and Pamela Paxton. 2000. "Subjective Measures of Liberal Democracy." *Comparative Political Studies* 33, 1: 58-86.

Bratton, Michael and Robert Mattes. 2001. "Support for Democracy in Africa: Intrinsic or Instrumental?" *British Journal of Political Science* 31: 447-474.

Burkhart, Ross and Michael Lewis-Beck. 1994. "Comparative Democracy: The Economic Development Thesis." *American Political Science Review* 88, 4: 903-910.

Carothers, Thomas. 2002. "The End of the Transition Paradigm." *Journal of Democracy*, 13, 1: 5-21.

Chee, Soon Juan. 2001. "Pressing for Openness in Singapore." *Journal of Democracy* 12: 157-167.

Collier, David and Robert Adcock. 1999. "Democracy and Dichotomies: A Pragmatic Approach to Choices about Concepts." *Annual Review of Political Science*, 2: 537-565.

Coppedge, Michael. 2003. "Book Review: Przeworski et al. 2000," *Studies in Comparative International Development* 38, 1: 123-126

Dahl, Robert. 1971. *Polyarchy.* New Haven: Yale University Press.

De Boef, Suzanna and Jim Granato. 1997. "Near-Integrated Data and the Analysis of Political Relationships." *American Journal of Political Science* 41, 2: 619-640.

_______ . 1999. "Testing for Cointegrating Relationships with Near-Integrated Data." *Political Analysis* 8 (1): 99-117.

Diamond, Larry. 1992. "Economic Development and Democracy Reconsidered," in *Reexamining Democracy: Essays in Honor of Seymour Martin Lipset*. Eds. Marks, Gary and Larry Diamond. Newbury Park: Sage Publications.

________. 1999. *Developing Democracy. Toward Consolidation*. Baltimore: The John Hopkins University Press.

________. 2002. "Thinking about Hybrid Regimes." *Journal of Democracy*, 13, 2: 21-35.

Elkins, Zachary. 2000. "Gradations of Democracy? Empirical Tests of Alternative Conceptualizations," *American Journal of Political Science* 44: 287-294.

Elster, Jon. 1998. *Deliberative Democracy*. Cambridge: Cambridge University Press.

Finkel, Steven. 1995. *Causal Analysis with Panel Data*. (Sage University Paper series on Quantitative Applications in the Social Sciences, 07-105). Thousand Oaks, CA: Sage.

Gasiorowski, Mark. 1995. "Economic Crisis and Political Regime Change: An Event History Analysis." *American Political Science Review* 89, 4: 882-897.

Gleditsch, Kristian and Michael Ward. 1997. "Double Take: A Reexamination of Democracy and Autocracy in Modern Politics." *The Journal of Conflict Resolution*, 41, 4: 361-383.

Goldsworthy, Jeffry. 2001. “Legislative Sovereignty and the Rule of Law,” in *Skeptical Essays on Human Rights*. Eds. Compbell, T., K.D. Ewing, and A. Tomkins. Oxford: Oxford University Press.

Griliches, Zvi.1967. “Distributed Lags: A Survey.” *Econometrica* 35, 1: 16-49.

Hadenius, Axel. 1992. *Democracy and Development*. Cambridge: Cambridge University Press.

Helliwell, John. 1994. “Empirical Linkages between Democracy and Economic Growth,” *British Journal of Political Science* 24: 225-248.

Hill, Kim Quaile. 1994. *Democracy in the Fifty States*. Lincoln: University of Nebraska Press.

Holden, Barry. 1974. *The Nature of Democracy*. London: Nelson.

Inglehart, Ronald. 1988. “The Renaissance of Political Culture,” *American Political Science Review* 82: 1203-1230.

Inglehart, Ronald. 1997. *Modernization and Postmodernization. Cultural, Economic and Political Change in 43 Societies.* Princeton; Princeton University Press.

_______. 2003. “How Solid is Mass Support for Democracy—And How Can We Measure It?” *PS: Political Science and Politics*, 36: 51-57.

Inglehart, Ronald et al. 2000. *World Value Surveys and European Value Surveys, 1981-1984, 1990-1993, 1995-1997*. ICPSR Study 2790. Ann Arbor: Inter-university Consortium for Political and Social Research.

Inglehart, Ronald and Christian Welzel. 2003. “Political Culture and Democracy: Analyzing Cross-Level Linkages.” *Comparative Politics* 36, 1: 61-79.

Karl, Terry Lynn. 1990. “Dilemmas of Democratization in Latin America,” *Comparative Politics* 23: 1-21.

Lipset, Seymour M. 1959. “Some Social Requisites of Democracy: Economic Development and Legitimacy.” *American Political Science Review* 53: 69-105.

Muller, Edward and Mitchell Seligson 1994. “Civic Culture and Democracy: The Question of Causal Relationships,” *American Political Science Review* 88, 3: 635-652.

Munck, Gerardo L. and Jay Verkuilen. 2002. “Conceptualizing and Measuring Democracy: Evaluating Alternative Indices,” *Comparative Political Studies* 35: 5-34.

O’Donnell, Guillermo. 1994. “Delegative Democracy.” *Journal of Democracy* 5: 55-69.

_______. 2001. “Democracy, Law, and Comparative Politics,” *Studies in Comparative International Development* 36: 7-36.

O’Donnell, Guillermo and Phillippe Schmitter. 1986. “Tentative Conclusions about Uncertain Democracies,” in *Transitions from Authoritarian Rule: Prospects for Democracy*. Eds. O’Donnell, G., P. Schmitter, and L. Whitehead. Baltimore and London: The Johns Hopkins University Press.

Pennock, Roland. 1979. *Democratic Political Theory*. Princeton: Princeton University Press.

Przeworski, Adam and Fernando Limongi. 1997. “Modernization: Theories and Facts.” *World Politics* 49: 155-183.

Przeworski, Adam, Michael Alvarez, José Antonio Cheibub and Fernando Limongi. 2000. *Democracy and Development: Political Institutions and Well-Being in the World, 1950-1990*. Cambridge: Cambridge University Press.

Putnam, Robert.1993. *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton: Princeton University Press.

Rose, Richard and William Mishler. 2001. “Comparing Regime Support in Non-Democratic and Democratic Countries,” *Democratization* 9, 2: 1-20.

Rustow, Danckwart. 1970. “Transitions to Democracy: Toward a Dynamic Model.” *Comparative Politics* 2: 337-364.

Schedler, Andreas. 2001. “Measuring Democratic Consolidation.” *Studies in Comparative International Development* 36: 66-92.

Sussman; Leonard.1982. “The Continuing Struggle for Freedom of Information.” in *Freedom in the World, 1982*. Ed. Gastil, Raymond. Westport: Greenwood Press.

Welzel, Chris, Ronald Inglehart, and Hans-Dieter Klingemann. 2003. “Human Development as a Theory of Social Change: A Cross-Cultural Perspective.” *European Journal of Political Science* 42, 3: 341-379.

Whitehead, Laurence (2001). “Bolivia and the Viability of Democracy.” *Journal of Democracy* 12: 6-16.

Copyright of Studies in Comparative International Development is the property of Transaction Publishers and its content may not be copied or emailed to multiple sites or posted to a listserv without the copyright holder's express written permission. However, users may print, download, or email articles for individual use.